

ON CHANGES IN ORNITHOLOGICAL NOMENCLATURE—A REPLY TO CRITICS.

BY LEONHARD STEJNEGER.

"Consistency is a jewel."—Dr. E. COUES.

A SHORT paper of mine, published about a year ago, entitled: 'On some generic and specific appellations of North American and European birds,'* was intended to furnish such data as might be properly considered in studying the nomenclature of North American and European birds. It has caused considerable comment, and two courteous editorials—one in the 'Bulletin of the Nuttall Ornithological Club' (VI, 1882, p. 178), and the other in 'The Ibis' (1883, p. 116)—have passed several remarks upon it, which make an answer from me desirable. My excuse for presenting a reply so late is that I have but recently returned from my journey to the Commander Islands and Kamtschatka.

The American reviewer admits that a principle, like that which I want rigorously enforced, is most likely to bring the now almost overpowering confusion to an end. He says: "We believe that the surest way out of the nomenclatural difficulties that beset us is to be found in some such simple rule as this, and that to upset every name that can be upset according to any recognized principle is really the shortest road to that fixity of nomenclature for which we now all sigh like furnaces." But nevertheless he thinks that there ought to be a statute of limitation, "by which a bird resting in undisturbed enjoyment of its name for, say, a century, or half a century, should not be liable to eviction under the common law of priority."

Now, in the first place I wish to emphasize that a law, may it be ever so good, will never bring the longed-for result, unless enforced rigorously; that is, *without exception*. If the law prohibiting changes of names which have been in use for fifty years, shall be applied to changes proposed by me, it must also be applied to changes proposed by other authors. If we take the two leading lists of North American birds, Coues's and Ridg-

* Proc. U. S. Nat. Mus. V, 1882, pp. 28-43.

way's, we shall find many names which will have to be given up. I will only mention a few examples, in order not to swell this paper, as everybody who is somewhat familiar with the subject can easily make considerable additions to the list. We then would have to drop:—

<i>Alle nigricans</i>	for	<i>Mergulus alle.</i>
<i>Simorhynchus pygmæus</i>	..	<i>S. camtschaticus.</i>
<i>Colymbus torquatus</i>	..	<i>C. glacialis.</i>
<i>Histrionicus minutus</i>	..	<i>Cosmonetta histrionica.</i>
<i>Cygnus columbianus</i>	..	<i>C. americanus.</i>
<i>Asio accipitrinus</i>	..	<i>A. brachyotus.</i>
<i>Pinicola enucleator</i>	..	<i>Corythus enucleator.</i>
<i>Pica rustica</i>	..	<i>P. caudata. etc.. etc.*</i>

What under such circumstances would be the fate of Bartram's names, such as *Elanus glaucus* (Bartr.) Coues, *Ictinia subcærulea* (Bartr.) Coues, *Corvus frugivorus* Bartr., *Spizella agrestis* (Bartr.) Coues, *Botaurus mugitans* (Bartr.) Coues, *Aramus pictus* (Bartr.) Coues, etc., etc.? Are we going to give up again Forster's names of 1772? And how about those of Philip Statius Müller restored by Cassin, or the numerous names of Boddaert?

Another question presents itself in this connection: How are we then going to deal with names that have for more than half a century been wrongly identified? Thus, for instance, to cite one of my proposed changes, *Totanus glottis*, which Coues still gives as "(L.) Bechst.," although it seems evident that Linneus's and Bechstein's *glottis* are two widely different birds? Some European authors, not long ago—and most certainly more than fifty years after Bechstein's mistake—adopted Gmelin's name *cinerascens*; but why not accept Gunnerus's name, which is older, better defined, and in every other respect at least just as good? "The long survival of an error does not justify its continued perpetuation after detection," says Dr. E. Coues (Check List, 2d ed., p. 24); and that is precisely my opinion, too.

The American reviewer thinks that a law as above is but just, as these early authors, whose nomenclature is forgotten, have not taken "the trouble to make good their title in due time." But whose fault is it that the names have been temporarily

* Not to speak of *Hydrochelidon lariformis*, which for other reasons is rejected in my paper.

out of use, theirs who died long ago, or that of writers of succeeding generations who have forgotten them? I think that every ornithologist is the heir of those authors, and has the right of claiming that justice be done to them. I confess, however, that I claim this justice not so much for the sake of the justice itself, or for the 'few departed greatnesses,' but simply because I feel convinced that this justice tends to the benefit of the science, and that the oldest name at last will be recognized, in spite of all efforts to keep it down. I am in this respect very fortunate in agreeing with Dr. Coues, who, in a reply to Mr. Allen about the restitution ('Fasti Ornithologiæ Redivivi,' in Proc. Phil. Acad., 1875, p. 338) of Bartram's names (of 1791)* says: "Mr. Allen inquires with some warmth, whether this sort of thing 'tends to the best interest of science.' It may or may not, I reply, but I believe it does, and that time will show it does. At any rate, the reason Mr. Allen adduces for his belief that it does not is not a sound one. He says, 'If the example Dr. Coues is here setting be followed, there will be no stability to our nomenclature for a long time, but only, except perhaps to a few experts, the most perplexing confusion.' *But I contend that the only possible road to stable nomenclature is that which leads to the very bottom of the matter.* In the nature of the case, the process of striking bed-rock is desultory, uncertain and confusing; I admit, as I deplore, the inconvenience and the difficulty. But a fact is no less a fact because it is a disagreeable one; and whether we like it or not, *the fact remains that names of species will continue to shift until the oldest one that is tenable according to rule is recognized.* [†] Therefore the sooner a species is hunted down, the better; . . . To speak my mind freely, I may add that I should have been disappointed, considering that I had signally failed, had not my paper made some disturbance; exactly that effect was anticipated and fully intended, otherwise the paper would not have shown *raison d'être*. I am encouraged further to believe that the paper took its own step, however short, in the right

* American Naturalist, X, 1876, pp. 100-101.

[† Mr. Allen's criticism, as the whole tenor of his article clearly shows, was directed not against necessary changes in nomenclature, nor against the rule of priority, or any other approved canon of nomenclature, but against the acceptance of names having no scientific basis, as was the case with most of the proposed restorations from Bartram. In his rejoinder to Dr. Coues he says: "The point at issue is not whether Bartram's identifiable, described, and binomially named species are entitled to recognition, for no one would be foolish enough to deny that" (Amer. Nat., X, p. 176).—J. A. A.]

direction, by the recollection that certain *Fasti* of my honored predecessor in his particular line of work, whose title I have had the presumption to revive, were received with wry faces and shrugs—and received, nevertheless. I am perfectly satisfied to let my own be tested in the crucible of time.” His words are written as out of my own heart, and fit my case like a glove. Dr. Coues’s innovations were also met with wry faces and shrugs—and received nevertheless, and this I trust will be the fate of my ‘innovations’ too.

Lastly, my esteemed critic asks if I have “in all cases taken up names which rest upon diagnoses,” and further, if “indication of a type species makes a generic name valid.”

As all the proposed changes of the specific names rest upon descriptions, most of them being for the time even very good, both the above questions refer to the generic appellations. As the second question is the more comprehensive, I take it first, and say that, at the present time at least, it is the usually followed rule to allow generic names, even if without diagnosis, when only their type can be ascertained. I could mention plenty of examples from Dr. Coues’s latest check-list, Ridgway’s list, Dresser’s list, British Ornithologists’ Union’s list, and probably from the greater part of authors. From the last mentioned list I will only cite one example, *Erithacus* Cuv., 1799-1800, as it is an exact counterpart of one of the least approved of my proposals, viz., *Urinator* Cuv. Not less opposition will meet the proposed substitution of Forster’s names of the Swallows for those of Boie. But *both of them, Boie as well as Forster*, give only types, no descriptions or diagnoses. It will in this connection be well to remember that in fact almost all of Boie’s genera rest only upon mention of the types without descriptions, and so do Brehm’s in ‘*Isis*,’ 1828; so do a great part of Bonaparte’s, Reichenbach’s, and Gray’s genera, besides plenty of others. Practically we may say the same about Kaup’s genera of 1829, and, in fact, about those of most of the old writers, as their diagnoses of the genera—as well as Linnæus’s—for a great part would be completely unrecognizable if not accompanied by typical species. I think that the question about the validity of genera has got the best answer in the fact that it in most cases, especially among the older authors, is easier to determine the identity of a genus name with type species only, than with diagnosis only.

Turning to my English reviewer, I want especially to call attention to the fact that there are many points in ornithological nomenclature wherein the English naturalists are compelled to disagree with a great many zoölogists, especially with those of North America, who, with only few exceptions, take Linnæus's 10th edition for their nomenclatural starting-point, while the former still strictly adhere to the 12th edition.

But there are two points in the 'Ibis' review which can be discussed with advantage, as they have nothing to do with the vexed question about the two Linnæan editions. In a foot-note the editors give their reasons why they feel justified in continuing the use of the universally adopted *Plectrophanes* for *Emberiza nivalis*, and not accepting for the latter the term *Plectrophenax* proposed by me. They say: "Although it is quite true that, in the preface to his Vögel Liv- und Esthland (1815), Meyer casually mentioned the term *Plectrophanes* as applicable to *Fringilla lapponica* ONLY, we find, on reference to the 'Zusätze u. Bericht. zu Meyer und Wolf's Taschenbuch,' 1822 (in which the genus was first properly characterized), that *Plectrophanes* was intended to include both *Fringilla lapponica* and *Emberiza nivalis*."

When Bechstein, in 1803, created the division *Calcarius* (a term also used by him in 1807, in the 2d edit. of his 'Gemeinn. Naturg. Deutschl.,' III, p. 245) he considered *lapponicus* and *nivalis* generically distinct. He included the former under *Calcarius*, for which genus *lapponicus* consequently is the type.* In his later books he followed the same practice. In 1810 Meyer and Wolf strictly followed the example of Bechstein, separating *lapponicus* from the body of the genus *Fringilla* as a separate 'family,' as they called it, still leaving *nivalis* under *Emberiza*. In 1815 Meyer, however, recognized *lapponicus* as a separate genus in the most binding words: "gehört keineswegs zu der Gattung Fringilla, sondern muss eine eigene Gattung bilden; ich nenne sie Plectrophanes, Spörner"; but he treats *nivalis* under *Emberiza*, thus evidently showing that *Plectrophanes* was NOT intended to include both *lapponica* and *nivalis*, as the Editors of 'The Ibis' state. It is moreover not correct to say that the genus was not properly characterized before 1822. Bechstein had already 'properly characterized' *Calcarius* in 1803, and we have seen that there cannot be the

* Authors regarding *nivalis* as being congeneric with *lapponicus* will therefore have to adopt the combination *Calcarius nivalis* (Lin.).

slightest doubt that *Plectrophanes* of 1815 is an unconditional synonym of *Calcarius*, 1803. In 1822, Meyer first included *nivalis* in the genus originally created for *lapponicus*; he still used *Plectrophanes*, although he of course knew Bechstein's name very well; but Meyer and the ornithologists of that date were not very scrupulous in that respect, changing old names very often only for the reason that they did not seem appropriate enough. However, the type of the genus *Plectrophanes* of 1822 is still *lapponicus*, and no interpretation can ever prove the contrary. Kaup, in 1829, first made *nivalis* the type of his *Plectrophanes*. We have here before us a case exceptionally clear; we have either to accept a new name, my *Plectrophenax*, or TO VIOLENTLY CHANGE THE TYPE OF A GENUS AGAINST OUR BETTER KNOWLEDGE. But where are we going if such a thing be allowed?

The critic in 'The Ibis' says that "excellent reasons may be found for rejecting *any* terms given by Schäffer, Gunnerus (!), and Hasselquist." The latter, of course, is not acceptable to ornithologists starting from 1766, but I am quite unable to see the "excellent reasons for excluding the other two, especially Gunnerus." (!)

I repeat what I said about Gunnerus, viz., that he wrote after 1766; he was a strict binomialist; the language he used for his descriptions was Latin; his descriptions and diagnoses are clear and well defined; he was at the time a man of high scientific standing, and recognized as a first-class naturalist; his different writings were well known and well studied by his contemporaries; and, finally, his botanical names are accepted and generally used in modern botany. I ask once more, Where are the 'excellent reasons' for his exclusion?

The same remarks are for the greater part applicable to Schäffer also. As examples of his diagnoses I quote those accompanying the names proposed by me to be revived.

One page 52 of his 'Museum Ornithologicum'* we find:—

* The full title of this book is: Museum | ornithologicum | exhibens | enumerationem et descriptionem | avium | quas | nova prorsus ratione sibi paratas | in museo suo | asservat | D. Jacobus Christianus Schaeffer | eccl. ev. Ratiob. past. superint. et ven. consist. ass. primar. | ser. et pot. regi Dan. Norv. a consiliis et professor. | Acad. imp. natvr. cvr. Petr. Lond. Berol. Upsal. Rob. Monac. et Mauntr. | soc. hist. Goett. bot. flor. patr. Svec. phys. Lond. Goth | soc. oecon. Cell. Bern. Lvs. Styr. Bvrgh. Lips. et plvr. Tevt. membrum | acad. scient. Paris, a litterarum commercio. | — | LII tabulae aevi incisae et coloribus distinctae. | — | Ratisbonae MDCCLXXXIX.

"168. *Tringa Merula*. Le Merle-d'eau. **Wasseramsel**.

Tringa superne fusco-nigricans; genis, gutture, collo inferiore et pectore niveis; ventre supremo fusco-rufescente, imo ventre, reetricibusque nigricantibus.

Mus. Schaeffer. No. 68.

Briss. Av. V. 252. 19. Le Merle d'eau.

Linn. S. N. 12. 290. 5. *Sturnus cinclus*.

Scop. Bemerk. n. 223. **Die Wasseramsel.**"

On p. 49 of the same work we find: —

"161. *Vanellus capella*. Le Vanneau. **Gybitz**.

Vanellus cristatus, superne viridi aureus, inferne albus; capite superiore nigro-viridante; crista nigra; taenia infra oculos nigricante; gutture albo; collo inferiore nigro viridante, pennis in apice albo fimbriatis; reetricibus decem intermediis prima medietate candidis, altera nigris, apice albido marginatis, utrinque extima candida, macula nigra interius insignata.

Mus. Schaeffer. No. 7.

Briss. Av. V. 94. I t. 8. f. 1. Le Vanneau.

Linn. S. N. 12. 248.2. *Tringa Vanellus*.

Frisch. Av. 213. *Vanellus*. **Kywitz**.

Schaeff. Orn. t. 69.

Naturf. XIII, St. p. 215. n. 122. **Der Kiebitz**.

Scop. Bemerk. n. 141. **Der Kybitz.**"

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A SECOND SEASON IN TEXAS.*

BY NATHAN CLIFFORD BROWN.

IN the winter of 1882-1883, the writer made a second visit to the village of Boerne, in Southwestern Texas, and devoted the ten weeks subsequent to January 27, 1883, to field work amongst the birds of the vicinity. Throughout this period the country presented an appearance very different from that familiar in 1880: instead of desolate expanses of bare earth, a green sward was almost everywhere to be seen; in the fields were rank growths of frost-killed weeds; and along the creek were patches of coarse grasses and even occasional little sedgy morasses. The creek itself, which during the season of 1880 only at long intervals accumulated a sufficient volume of water to flow with an

* See Bull. Nutt. Orn. Club, Vol. VII, pp. 33-42.